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June 4, 1996

**Ex Parte**

William F. Caton
Federal Communications Commission
19191 M Street, N.W. Rm. 222
Washington, D.C. 20554

**Re: LEC Price Cap Regulation, CC Docket 94-1**

Dear Mr. Caton:

Please find attached a Declaration by Melvin A. Fuss, prepared on behalf of Bell Atlantic, in response to new arguments raised by AT&T and Ad Hoc Telecommunications Users Committee in their reply comments in the above captioned proceeding. Specifically, this paper demonstrates that the claims of the existence of an input price differential, and the possibility of calculating total factor productivity growth for interstate services alone, are flawed. In addition, this declaration further supports Dr. Fuss' original declarations in the this proceeding.

An original and two copies of this ex parte notice and attachment and diskettes are being filed today. Please include this letter, the attached declaration and the diskette into the record as appropriate.

Sincerely,

Attachment

cc w/o disk: J. Farrell, G. Rosston, A. Bush, S. Spaeth, R. Metzger, L. Selzer, L. Huthoefer, J. Jackson

cc: w/disk: ITS

No. of Copies rec'd 0+2
List ABCDE

## CERTIFICATE OF SERVICE

I, Maureen Keenan, do hereby certify that on this 4th day of June, 1996, a copy of the foregoing Ex-Parte together with the attached declaration and diskette, were mailed by U.S. first class mail, postage prepaid, to the parties listed below:

James Blaszak
Levine, Blaszak, Block & Boothby
1300 Connecticut Avenue, Suite 500
Washington, D.C. 20036

Peter Jacoby
Attorney for AT&T Corp.
AT&T
295 North Maple Avenue
Basking Ridge, New Jersey 07920

**Before the**
**FEDERAL COMMUNICATIONS COMMISSION**
**Washington, D.C. 20554**

| | | |
|---|---|---|
| **In the matter of** | ) | |
| | ) | |
| **Price Caps Performance Review** | ) | **CC Docket 94-1** |
| **for Local Exchange Carriers** | ) | |

# DECLARATION OF MELVYN A. FUSS

**I, Melvyn A. Fuss, declare the following:**

## Introduction

1. In this declaration, I respond to new arguments raised by AT&T and the Ad Hoc Telecommunications Users Committee in their reply comments regarding their claims of the existence of an input price differential, and the possibility of calculating total factor productivity growth for interstate services alone. I demonstrate that these new arguments are flawed. As a result, the Commission may continue to rely on the conclusions in my original declarations that there is no permanent input price differential and that there can be no economically meaningful calculation of interstate-only TFP growth. In this declaration, I specifically demonstrate the following:

(1) Drs Norsworthy and Berndt's attacks (on behalf of AT&T) on my evidence that there is no permanent input price differential is, in reality, an attack on the methodology of Bush and Uretsky, adopted by me for the purpose of my analysis.

Regardless, the Norsworthy/Berndt argument relies on an improper application of the appropriate test. The correct procedure in fact supports the conclusion that there is no permanent input price differential.

(2) Drs Norsworthy and Berndt's attempt to justify the calculation of interstate-only TFP growth is based on an algebraic error which undermines their entire argument.

(3) Dr Nadiri offers only unsupported arguments in favor of AT&T's position that it is possible to calculate an economically meaningful interstate-only TFP growth rate. Indeed, he relies on a published paper of mine which contradicts one of his main arguments.

(4) ETI (on behalf of Ad Hoc), in an attempt to rebut my hypothesis testing results, supresses evidence which is inconsistent with its argument that there is a permanent input price differential. As a result, ETI's arguments are devoid of any legitimate economic meaning.

## A. Response to Reply Statement of Dr. John R. Norsworthy and Dr. Ernst R. Berndt on Behalf of AT&T

2. In this section of my response I consider the Reply Statement of Drs. Norsworthy and Berndt which appears as Appendix B to Reply Comments of AT&T, March 1, 1996. I will concentrate on Drs. Norsworthy and Berndt's evaluation of my initial declaration regarding the input price differential, and on their attempts to justify the calculation of interstate service - specific TFP growth rates.

1. The Input Price Differential

3. Norsworthy and Berndt's use of times series procedures to criticize my analysis is actually an attack on the Bush-Uretsky methodology and the Bush-Uretsky regression equations. To the extent their analysis has any validity, which overall it does not, it discredits the Bush-Uretsky regression equations which appear in Appendix F of the FCC's Performance Review Order. It is logically impossible for Norsworthy and Berndt to continue to extol the virtues of the Bush and Uretsky equations while at the same time trying to discredit the equations in my analysis which use the same methodology. Indeed, Norsworthy and Berndt test the exact equations estimated by Bush and Uretsky, and find Bush and Uretsky's results to be spurious!

4. The essence of the Norsworthy-Berndt critique of the Bush-Uretsky methodology is their claim that the Bush-Uretsky equations (or the variations that I estimate) suffer from a basic problem sometimes encountered when using data drawn from a series of yearly observations. This basic problem is that even if some variables are truly unrelated to one another, a regression analysis will make it appear as if those variables were highly correlated with one another. Econometricians call such regression results spurious. When the regression results are spurious, any attempt to base conclusions on such regression equations are meaningless. Norsworthy and Berndt claim that the Bush-Uretsky regressions involving the input price differential are spurious regressions. To the extent their claim is correct, no implications concerning the input price differential can be drawn from such

regressions.

5. Fortunately for Bush and Uretsky (and myself), the Bush-Uretsky methodology survives the attack by Norsworthy and Berndt because Norsworthy-Berndt apply their time series procedures incorrectly in their tests.[1] The proper inference, when the Norsworthy-Berndt tests are carried out correctly, is that the regression results which use the LEC input price as the dependent variable may be spurious, and hence should be treated with suspicion. However, the regression results which use the LEC-U.S. input price *differential* as the dependent variable are not subject to the problems associated with spurious regressions, and remain valid despite Norsworthy and Berndt's claims to the contrary. The results based on these equations support the conclusion that the input price differential was a temporary phenomenon. The technical details of my critique of the Berndt-Norsworthy time series analysis is contained in Appendix A.

6. Norsworthy and Berndt at various times in their statement complain about the data I use and attempt to discredit my analysis on the basis that these data are suspect. They seem particularly concerned about the Moody bond yield (page 20). For some reason these complaints do not extend to the Bush-Uretsky analysis. This is exceedingly strange. All my data for the 1949-92 period were taken directly from the Bush-Uretsky data base as it appears in Appendix F. It is the identical data. AT&T was informed of this fact in a Bell Atlantic ex parte sent to AT&T on February 20,

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[1] Norsworthy and Berndt ignore a warning in the manual of the computer program they use (Time Series Processor (TSP) 4.3) that their cointegration test procedures are not valid procedures when a certain necessary prior mathematical test (called the unit root test) is not met This failure occurs in one-half of their tests.

1996. The Moody bond yield data were introduced into the analysis by Bush and Uretsky - it is their series, not mine.[2]

2. Interstate Access Service Specific TFP

7. In my reply declaration in this proceeding, I demonstrated that it is not possible to calculate an economically meaningful separate TFP growth rate for interstate access services. In section C of their Statement, Norsworthy and Berndt derive an equation (equation (3), page 33) which they claim demonstrates how to separate TFP growth for interstate services from that for other services. In describing this equation they claim "this manipulation is algebraically valid."(page 33).

8. In fact, the manipulation is not algebraically valid. Equation (3) is based on their equation (2), which contains a basic algebraic error that completely invalidates equation (2), and hence also invalidates equation (3). The specific algebraic error is described in detail in Appendix B.

9. Equation (3) is the cornerstone of Norsworthy and Berndt's attempts to justify the existence of a TFP growth measure for interstate access services. Since this equation is shown to be invalid as a matter of basic algebra, their case for an economically meaningful service-specific TFP measure is without analytical support.

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[2] Bush and Uretsky give as their source of the Moody bond yield data The Economic Report of the President, 1994. Perhaps Norsworthy and Berndt's confusion arises from the fact that Bush and Uretsky mislabelled the series they used. They actually used the Moody Corporate Aaa bond yield rather than the public utility bond yield.

B. Response to the Statement of Dr. M. Ishaq Nadiri on Behalf of AT&T

10. In this section of my response I consider the Statement of Dr. M. Ishaq Nadiri which appears as Appendix C to Reply Comments of AT&T, March 1, 1996. In his statement Dr. Nadiri attempts to support AT&T's position that there exists an economically meaningful calculation of interstate access service-specific TFP growth rates. In fact, Dr. Nadiri presents no evidence which explains how an economically meaningful service-specific TFP growth measure could be calculated. His argument consists of three parts: (1) a repeat of the AT&T position that the FCC's cost allocation rules are economically meaningful, (2) a discussion of cost elasticities for toll and local services obtained from Canadian studies of Bell Canada data, and (3) a discussion of cost complementarity.[3]

11. In this response I will concentrate on the second and third pieces of evidence.[4] The fact that the FCC's cost allocation procedures cannot be used to obtain economically meaningful service-specific TFP growth measures has been thoroughly documented elsewhere in these proceedings

12. Dr. Nadiri makes two points with respect to estimates of cost elasticities. He first claims that since the cost elasticity of toll service is smaller than the cost

[3] Two outputs are said to be cost complements when an increase in the volume of one output decreases the marginal cost of the other output.

[4] The second piece of evidence is discussed below. The third piece of evidence is discussed in Appendix C. In that appendix I show that Nadiri's discussion of cost complementarity reaches an incorrect conclusion and therefore provides no support for the concept that a separate TFP growth rate can be calculated for an individual service.

elasticity of local service in two cited Canadian studies[5], this is evidence of increased efficiency of switched access service. This claim is incorrect. The relative levels of local and toll cost elasticities bear no relation to the question of relative efficiency growth rates. If this linkage were true, it would mean that smaller firms (i.e, those with smaller levels of inputs) would necessarily exhibit lower rates of TFP growth than larger firms that used larger levels of inputs This of course makes no sense. Dr. Nadiri has mixed up absolute levels and growth rates.

13. The second claim is that because the cost elasticity of toll service was observed to decline faster than the cost elasticity of local service, this is evidence of relatively faster TFP growth in toll services Nadiri offers no substantive support for the existence of a direct link between a decline in a service-specific cost elasticitity and a measure of service-specific TFP growth. In fact, no such direct link exists, nor could it, since service-specific TFP growth is not a meaningful concept.

14. The evidence quoted by Nadiri that the decline in the toll elasticity was faster than the decline in the local elasticity is based on an unpublished memo (Nadiri and Nanda (1995)). This memo was not included with Dr Nadiri's statement and therefore cannot be evaluated. Nadiri's evidence is contradicted by my paper (Denny, Fuss and Waverman (1981)) which was relied upon by Nadiri to support an earlier claim. In my paper the opposite result occurs: the local cost elasticity declines more

[5] The cost elasticity estimates in Bernstein (1989) are unreliable because of the peculiar specification of technical change which he employed that led to the following estimated empirical results for Bell Canada: During 1954-57 there was no technical change. In 1958 there was technical progress of 19%. During 1958-70 there was no technical change. In 1971 there was technical regress of 19%. During 1971-78 there was no technical change. Cost elasticity results which depend on this pattern of technical change cannot be taken seriously.

rapidly than the toll cost elasticity over the 1952-76 period[6].

15. I conclude that Dr. Nadiri has not presented any objective evidence which would point to the possibility of measuring TFP for interstate access services in an economically meaningful way. As I have stated earlier, it is impossible conceptually to find a method of measuring TFP for interstate services alone unless interstate services uses no inputs in common with intrastate services. This is clearly not the case. The input costs which are shared between interstate and intrastate services cannot be allocated in a an economically meaningful manner. Such an allocation is a precondition to economically meaningful estimation of TFP for interstate services alone, given the joint nature of the production process.

## C. Response to the Reply Statement of Economics and Technology Inc. on Behalf of the Ad Hoc Telecommunications Users Committee

16. In this section, I consider the Reply Statement of Economics and Technology Inc. (ETI), which appears as an Attachment to Reply Comments of the Ad Hoc Telecommunications Users Committee, March 1, 1996. I will concentrate on ETI's evaluation of my initial declaration regarding the input price differential.

17. In my initial declaration, I demonstrated that the data used by Bush and Uretsky in Appendix F favoured the hypothesis that the LEC-US input price differential was a temporary phenomenon which ended around 1990. The hypothesis

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[6] Since Nadiri took the time to calculate average toll and local cost elasticities from the twenty-five yearly elasticites which appear in table 8 of my paper, it is surprising he did not see that the trend in these elasticities contradicted his own results.

adopted by Bush and Uretsky (and ETI), that an input price differential should be a permanent feature of a price caps plan for the LECs, was consistently rejected by the data in favour of the temporary change hypothesis. ETI contests these conclusions. It does not suggest that there is any error in my procedures. Rather, it claims that when the 1990 data point is dropped from the data set used in the analysis, the temporary change hypothesis is rejected by the non-nested hypothesis testing procedure. ETI then concludes that the permanent change hypothesis is the preferred hypothesis. ETI justifies the exclusion of this data based on the claim that the 1990 data point is an "outlier". ETI's conclusions cannot withstand scrutiny.

18. ETI's conclusions are based on a series of flawed procedures and a presentation of evidence which is blatantly selected to produce desired results.

19. In ETI's reply (tables A10 and A11) they argue that, when a part of my testing procedure[7] is applied to the equation with the LEC input price growth rate as the left hand side variable, using the Christensen 1 data set with the 1990 data point deleted, the temporary change hypothesis is rejected

20. There are a number of errors in ETI's tables A10 and A11 which render ETI's entire analysis incorrect. First, ETI applies only one-half of the non-nested hypothesis testing procedure which must be applied in a correct application of the procedure. They incorrectly exclude the portions of the procedure which generate test

[7] In my initial declaration I used two testing procedures, the J- Test and the Cox Test. ETI restricts its reply arguments to an analysis of the Cox Test

results that reject their preferred conclusions.[8] Second, ETI applies the test to a regression equation which Norsworthy and Berndt, in their comments on behalf of AT&T, argue is spurious.[9] Third, ETI provides no argument as to why the 1990 data point should be considered an outlier, other than it is an inconvenient data point for someone who believes in the permanent change hypothesis. Econometricians have developed an objective analysis of outlier data, denoted the theory of influential outliers. When this theory is applied to the Christensen 1 data set used in tables A10 and A11 of the ETI submission, the 1990 data point is shown not to be an outlier [10]

21. In its analyses of the regression equations submitted by NERA and Lincoln Telephone (tables A2-A9), ETI always presents two versions of the regression equation; one with the LEC input price growth rate as the left hand side variable, and a second version with the LEC-US input price *differential* growth rate as the left hand side variable. When we come to tables A10 and A11 (the Cox Tests), the second version of the regression equation is suddenly missing. This inconsistency can perhaps be explained by the fact that the test results applied to the second version of the regression equation for the 1949-92 period are inconvenient to ETI. Even with

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[8] They perform the part of the testing procedure where the temporary change hypothesis is the null hypothesis. Incorrectly, they do not perform the part of the testing procedure where the permanent change hypothesis is the null hypothesis. The Cox Non-Nested Hypothesis Test consists of both tests. For the regression equation and data ETI uses, this second part of the test results in a rejection of the permanent change hypothesis, a result which ETI perhaps finds inconvenient. For details please see Appendix D of this response.

[9] They ignore the equation which is not subject to the spurious regression criticism, the equation with the LEC-US input price differential as the left hand side variable

[10] This result is based on a test which identifies outliers and is one of the testing procedures contained in the theory of influential outliers. The details of applying the theory of influential outliers to the 1990 data point is contained in Appendix D

the inappropriate deletion of the 1990 data point, the temporary change hypothesis is accepted for this period, while the permanent change hypothesis is rejected[11]. The blatant selection of the evidence by ETI in an attempt to support its conclusions is totally unacceptable economic analysis.

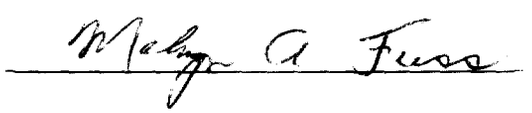
22. In conclusion, ETI's attempt to discredit the validity of the temporary change hypothesis through the use of the non-nested hypothesis testing methodology fails on a number of grounds. They consistently and without explanation supress evidence that contradicts their arguments. As a result they offer no economically meaningful argument.

## Reference

Judge, G.G, R.C. Hill, W.E. Griffiths, H. Lutkepohl, and T-C Lee (1988), Introduction to the Theory and Practice of Econometrics (second edition), John Wiley and Sons, New York.

[11] In this case, as in all other cases when the 1990 data point is deleted, the permanent change hypothesis is rejected. For a *complete* presentation of the test results see Appendix D.

I declare, under penalty of perjury, that the foregoing is true and correct.
Executed on May 31, 1996



Melvyn A. Fuss

## Appendix A

### Norsworthy and Berndt's Time Series Analysis of the Bush-Uretsky Methodology

The essence of the Norsworthy-Berndt critique of the Bush-Uretsky methodology is their claim that time series analysis (known as cointegration tests) of the regression equations used by Bush and Uretsky and myself lead to the conclusion that all of the regression results are spurious.

In this appendix I make two main points. First, I demonstrate that the time series analysis of Norsworthy and Berndt is in fact an attack on the Bush-Uretsky methodology. This point is made most vividly by the fact that Norsworthy and Berndt reject as spurious regressions that are identical to those estimated by Bush and Uretsky[12].

Second, I show that one-half of the cointegration tests of Norsworthy and Berndt are applied incorrectly. They incorrectly apply econometric principles and ignore warnings contained in the manuals of the very computer program they utilize.

### (a) Norsworthy and Berndt Reject the Bush-Uretsky Appendix F Results as Spurious

In tables A-7 to A-10 of their Reply Statement Norsworthy and Berndt use the Engle-Granger Cointegration Tests to test the residuals from what they call the "Fuss model" for unit roots. Of course it is not the Fuss model they test, but rather the Bush-Uretsky model, where some of the equations were modified by me by changing the form of the divestiture dummy variable. For clarity, it is useful to concentrate on the following equations which Norsworthy and Berndt label:

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[12] For obvious reasons, Norsworthy and Berndt choose to ignore this fact.

(1). Model: CPT C CPE DIVEST MOODY (Table A-7)
(2). Model: CPT C CPE D84 MOODY (Table A-7)
(3). Model: CPDIFF C DIVEST MOODY (Table A-7)
(4). Model: CPDIFF C D84 MOODY (Table A-8)
(5). Model: NPT C NPE D84 MOODY (Table A-9)
(6). Model: NPDIFF C D84 MOODY (Table A-10)

From Tables A-7 to A-10 it can be seen that the unit root hypothesis for models (1)-(6) cannot be rejected at the conventional 5% significance level used by Norsworthy and Berndt. This fact is noted by Norsworthy and Berndt (page 24). Hence according to Norsworthy and Berndt, these models are not cointegrated, and "[t]he inferences from those regressions are therefore likely to be invalid." (page 23)

Models (1) and (3) are the exact Bush-Uretsky equations estimated using the Bush-Uretsky data[13]. Models (2),(4),(5) and (6) are also the exact Bush-Uretsky equations, where the name of the divestiture dummy variable has been changed from DIVEST to D84. However, DIVEST and D84 are the exact same variables. This fact can be verified by comparing the cointegration statistics for models (1) and (2) in table A-7. They are identical. The cointegration statistics for model (3) from table A-7 and model (4) from table A-8 are also identical for the same reasons.

Since Norsworthy and Berndt have found that the Bush and Uretsky equations are not cointegrated, logically they cannot disagree with the following adjustment I have made to their summary statement on page 25: "In summary, the equations that Bush and Uretsky estimate are, by standard statistical criteria, inappropriate for their intended use: inference

[13] Strictly speaking, the Bush-Uretsky data only cover the periods 1949-1992 and 1960-1992 whereas Norsworthy and Berndt include the 1993 data point in their regressions. However, excluding the 1993 data point has no effect on the cointegration tests of models (1)-(6).

about the shift in the input price differential. ... The results Bush and Uretsky obtained, therefore, contribute nothing to our understanding of the input price differential"[14].

(b) One-Half of Norsworthy and Berndt's Cointregration Tests Are Incorrect

The test statistics used to test for cointegration require that the dependent variable and at least one of the independent variables be integrated of order one (I(1)). When this condition is not met, test statistics such as the Dickey-Fuller statistic used in the Engle-Granger (tau) Test[15] have unknown distributions and any attempt to test for the absence of cointegration is meaningless.

This requirement is made clear in the manuals which accompany the computer program TSP 4.3 used by Norsworthy and Berndt to perform their cointegration tests.

> "The cointegration of time series is a methodology for the analysis of time series pioneered by Engle and Granger (1987). Two or more series are said to be *cointegrated* if a linear combination of them is I(0) (is stationary or has all roots inside the unit circle) even though individually they are each I(1). Thus the hypothesis of cointegration consists of two parts: tests for I(1) of the individual series and I(0) of a linear combination. Usually the term cointegration testing refers only to the second part of the hypothesis; the test is performed *conditional* on the fact that each component series is I(1)." (italics in original, underlining emphasis added)
>
> User's Guide, TSP Version 4.3, March 1995, page 94.

[14] Contrast this statement with their earlier statement regarding the Bush-Uretsky regression results: "Our conclusion thus confirms the findings of Commission economists Bush and Uretsky,..." (page 6). Clearly the left hand does not acknowledge what the right hand is doing.

[15] This is the test used by Norsworthy and Berndt

> "The Engle-Granger Test is only valid if all the cointegrating variables are I(1); hence the default option to perform unit root tests on the individual series to confirm this before running the Engle-Granger test." (emphasis added)
>
> Reference Manual, TSP Version 4.3, May 1995, page 40.

Norsworthy and Berndt perform the required unit root tests on the individual series in tables A-5 and A-6. The series CPDIFF and NPDIFF ( the LEC-US input price differential growth rates) fail the unit root tests at the 5% significance level used by Norsworthy and Berndt[16]. This fact is acknowledged by Norsworthy and Berndt on page 23: "Unit root tests of the variables... reveal relatively high probabilities of unit roots for all variables except for CPDIFF and NPDIFF..." Hence the series CPDIFF and NPDIFF are not I(1) as required, and cointegration tests involving these variables are invalid (see above quotes from the TSP manuals). Tables A-8 and A-10 of the Norsworthy and Berndt reply, which involve CPDIFF and NPDIFF, are filled with invalid tests. As a result, one-half of the tests done by Norsworthy and Berndt are performed incorrectly

Since CPDIFF and NPDIFF are stationary variables, the regressions using these variables as dependent variables are not subject to the claim that the results might be spurious. They remain legitimate regressions and can be used to test whether the temporary or permanent change hypothesis best represents the input price differential data.[17]

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[16] The p-values of the tests are .01 and .0008 respectively.

[17] The right hand side variables (including the dummy variables DIVEST and D90) are I(1). While the presence of unit roots implies that these variables are non-stationary, the asymptotic test statistics remain valid. This occurs because it is reasonable to assume that the right hand side variables remain fixed in repeated samples, since none of them are trending without limits.

## Appendix B

### Analysis of the Norsworthy-Berndt Attempt to Define a Measure of TFP Growth for Interstate Access Services

In section C, pages 32-33 of their Statement, Norsworthy and Berndt derive an equation (equation (3)) which they claim demonstrates how to separate TFP growth for interstate services from that for other services. Equation (3) is based on equation (2) (page 32), which contains a basic algebraic error that completely invalidates equation (2) and hence also invalidates equation (3) and their ultimate conclusion.

The first part of equation (2) contains the statement

$$\Delta TFP_{ALL\ SERVICES} = \Delta(Y_C/X_C) = \Delta Y_C/\Delta X_C \qquad (B1)$$

where the subscript C indicates a total company variable.

While the first equality is just the definition of the growth in total company TFP from year t-1 to year t, the second "equality" represents a fundamental error in algebra; the two expressions are not equal. This can be seen from a simple numerical example. Suppose we obtained the following data on aggregate output $Y_C$ and aggregate input $X_C$ for two years.

| Time Period | Aggregate Output $Y_C$ | Aggregate Input $X_C$ |
|---|---|---|
| | | |
| Year 1 | 200 | 100 |
| Year 2 | 500 | 200 |

$$\Delta TFP_{ALL\ SERVICES} = \Delta(Y_C/X_C) = [500/200 - 200/100] / [200/100] = 0.25$$

Now consider the calculation of the right hand side of (B1).

$$\Delta Y_C/\Delta X_C = [(500-200)/200] / [(200-100)/100] = 1.50$$

Clearly $\Delta(Y_C/X_C) \neq \Delta Y_C/\Delta X_C$ and equation (2) of Norsworthy and Berndt is an invalid construction. Since equation (3) depends on equation (2) for its validity, it is also invalid. Hence Norsworthy and Berndt have not demonstrated the analytical possibility of calculating TFP for interstate access services in an economically meaningful way.[18]

[18] For the numerical example contained in the table, the company TFP growth rate between the two years is (.25)*100 = 25%. If Norsworthy and Berndt's equation (2) had been used in the calculation, the company TFP would have been estimated as (1.50)*100 = 150%, a large overestimate of the correct value. If the more conventional "difference in logarithms" method had been used to calculate the growth rates, the two TFP growth calculations would have been 22% and 132% respectively

## Appendix C

### Cost Complementarity and the Calculation of TFP Growth for Interstate Services

Dr. Nadiri introduces the idea that if the degree of local cost complementarity between the two services is constant, then their costs are not joint and separate TFP growth rates can be calculated for each service (pages 14-15 of his statement). This idea is incorrect.

Local cost complementarity occurs between two outputs if a small increase in the supply of one output reduces the marginal cost of the other output. Consider the following simple example of a joint cost function for two outputs ($y_1$ and $y_2$):

$$C(y_1, y_2) = \alpha_1 y_1 + \alpha_2 y_2 + \alpha_{11} (y_1)^2 + \alpha_{22} (y_2)^2 + \alpha_{12} y_1 y_2 \qquad (C.1)$$

where C is total cost and $\alpha_1, \alpha_2, \alpha_{11}, \alpha_{22}$ and $\alpha_{12}$ are parameters of the cost function. The term which measures local cost complementarity is $\alpha_{12} y_1 y_2$, since if this term did not exist, costs would be non-joint, and a conceptually meaningful cost allocation could be accomplished.[19]

Nadiri does not define what he means by "the degree of local cost complementarity". Local cost complementarity is usually calculated as $\partial^2 C / \partial y_1 \partial y_2$, and I will assume this is what he meant by degree of local cost complementarity. For the example developed in this footnote, this derivative is equal to $\alpha_{12}$, which is a constant. This fact can be seen as follows.

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[19] Costs allocated to output one would be $\alpha_1 y_1 + \alpha_{11} (y_1)^2$ and costs allocated to output two would be $\alpha_2 y_2 + \alpha_{22} (y_2)^2$.

Marginal cost for output one is calculated as

$$\partial C/\partial y_1 = \alpha_1 + 2\,\alpha_{11}\,y_1 + \alpha_{12}\,y_2 \qquad (C.2)$$

The degree of local cost complementarity is thus calculated as

$$\partial/\partial y_1\,(\partial C/\partial y_1) = \partial^2 C/\partial y_1 \partial y_2 = \alpha_{12}$$

The degree of local cost complementarity is constant, but costs are joint (unless $\alpha_{12} = 0$, the case of zero cost complementarity and non-joint production) and separate TFP growth rates for the two services cannot be calculated in an economically meaningful way. Hence Nadiri's statement that a constant degree of local cost complementarity implies non-joint costs is incorrect.

## Appendix D

### Analysis of ETI's Claim That the 1990 Data Point Should Be Dropped From the Sample

ETI claims that the 1990 data point is an outlier and hence should be dropped from the sample. There are two classifications of outliers which are possible. First, a data point may be an outlier independent of the model being estimated. This would occur if it were known that the data point had been calculated incorrectly, or recorded in error, or there was a change in the basic underlying data generating process which made that data point non-comparable with the rest of the sample. Second, a data point may be an outlier *relative to a particular model*. This would occur if the model had difficulty explaining that particular data point.

There is no evidence in these proceedings that the 1990 data point satisfies the first definition of an outlier. (ETI does not even make this claim.) Hence we need to explore the possibility that it satisfies the second definition; and if it does, determine the implications for testing the temporary change hypothesis versus the permanent change hypothesis.

Econometricians have developed a procedure to test for the possible existence of model-related outliers. It is known as the theory of influential outliers.[20] There are a number of stages to the testing procedure. First, using a specific model, a regression is performed with the potential outlier deleted from the sample. Second, the residuals from this regression are tested for normality. Third, conditional on acceptance of normality, a studentized residual is formed for the potential outlier and this residual is compared with the

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[20] An accessible description of this theory and the resulting test procedures can be found in the econometrics textbook Judge et al. (1988), chapter 22.

value of 2. If the residual is greater than 2, the observation is classified as an outlier, otherwise it is not an outlier.[21]

If the observation is not an outlier, the process terminates - there is no evidence the observation should be deleted from the sample. If the observation is an outlier, it is analyzed further to see whether it is influential. In the current context, an outlier will be influential if dropping it from the sample changes the decision regarding the choice between the temporary and permanent change hypotheses.

When testing competing hypotheses using the non-nested hypothesis testing procedure, it is possible that a data point will have a studentized residual greater than 2 conditional on one hypothesis, and less than 2 conditional on the other hypothesis. In such a case the data point cannot be considered an outlier, since to do so would bias the test in favour of the hypothesis for which the studentized residual is greater than 2. Under these circumstances, a data point is considered an outlier only if it has studentized t values greater than 2 conditional on both competing hypotheses.

In conducting the tests I will use the Christensen 1 data set used by ETI in its critique of my analysis. I will also analyze the Cox tests since these are the only ones considered by ETI. Finally, I will present results for both the case where CPT is the dependent variable and where CPDIFF is the dependent variable. Recall that the regression where CPT is the dependent variable (the only regression considered by ETI) may be spurious and so results from that regression should be viewed with suspicion.

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[21] Judge et al (1988), page 894, express this test as follows: "Studentized residuals that have values that could reasonably come from a t-distribution, say less than 2 in absolute value, are regarded as acceptable in terms of the model specification. Others are regarded as outliers."

Table D-1 contains values of the Bera-Jarque statistic used to test for normality of the errors.[22] This statistic is chi-squared with two degrees of freedom under the null hypothesis of normality. Hence the 5% significance level critical value is 5.99. Since all the values in Table D-1 are less than 5.99, normality is accepted and we can continue with the testing procedure. Table D-2 contains values of the studentized residuals for 1990. The studentized residuals are greater than 2 for the permanment change hypothesis, but less than 2 for the temporary change hypothesis. We are in the situation discussed above, where to claim that the 1990 data point is an outlier would be to bias the non-nested hypothesis test in favour of the permanent change hypothesis. Hence the 1990 data point is not an outlier, and the ETI procedure of dropping this data point from the analysis is invalid. The conclusion in my first declaration, that the permanent change hypothesis is rejected by the data whereas the temporary change hypothesis is not rejected, continues to be valid.

In spite of the fact that it is not valid to drop the 1990 data point from the analysis, I now go on to consider the effects of doing so on the non-nested testing results. I carry out this invalid procedure to demonstrate the selectivity bias which permeates ETI's criticism of my earlier analysis.

Tables D-3 and D-4 present the complete set of test results for the Christensen 1 data set through 1992 and 1993 respectively. These tables preserve the format of tables A.5 and A.6 of my initial declaration. The numbers with asterisks are the test results reported by ETI in their tables A10 and A11.

A consideration of the entries in my tables D-3 and D-4 demonstrate how selective

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[22] See Judge et al. (1988), pages 890-92 for a description of this test statistic.